Review of "Impact of pollution controls in Beijing on atmospheric oxygenated volatile organic compounds (OVOCs) during the 2008 Olympic Games: observation and modeling implications," Liu et al., ACP (2014)

Summary

As the title suggests, this paper examines the response of oVOC to large-scale emission reductions implemented in Beijing during the summer Olympics. This event was a prime experimental opportunity, and despite a number of other papers having been published on this topic, the current manuscript offers some new and unique elements. In particular, the use of a neural network to separate the effects of meteorology and emissions reductions on oVOC changes is a novel approach that sidesteps the need to explicitly model various processes.

The subject matter is appropriate for ACP. The English is fair but could be improved in places. I recommend this paper for publication in ACP after consideration of the following minor revisions.

General Comments

Section 3.3: It was not clear to me, at the outset, how the discussion of emission ratios tied in to the previous discussion of control measures. On a second glance I see the phrase "source patterns," but this is somewhat ambiguous. It should be made clearer in the first paragraph that the goal of this analysis is to determine whether the controls, in addition to changing the total emission amounts, also altered the *relative* emission of OVOC precursors.

Specific Comments

P26132, l4: impacts have also been evaluated from satellite observations. I suggest adding a few references for this aspect as well.

P26137, l10: It is stated that the neural network adequately reproduces the validation data, but I think this needs to be shown – perhaps as some scatter plots in the supplement. At the very least, it needs to be quantified, e.g. as a % accuracy.

P26145, l26: The Li et al. (2013) paper speculated that aerosol sinks *might* be important for HCHO, but they did not prove it. This sentence should be modified to reflect the distinction.

P 26145: On the same topic, the overprediction of HCHO, even with "corrected" deposition and dilution, is staggering. The authors suggest that sinks are to blame, but what about sources? If OH is overpredicted by 30% as suggested by Fig. 6a, this should have a marked effect on oVOC. An additional model simulation using "calculated" OH profile would provide a sensitivity test for this.

P26146, l4-10: What kind of sinks would be consistent with MVK+MACR only being over-predicted in the afternoon? Also, I would not say that the model predicts “nocturnal productions,” but rather that it does an OK job of representing nocturnal sinks (which presumably are mostly deposition?).

P26146, l26: Is the primary source of acetone associated with combustion? If not, it is probably not appropriate to use CO as the normalization factor for incorporating emissions. It might be more appropriate to use a constrained NMHC that comes from the same source.

P26149, l10: why are the modeled changes in acetone so big? Is it due to one particular class of VOC, and does this imply that the model is misrepresenting secondary acetone production?

Technical Comments

P26130, l22: suggest splitting this into two sentences.

P26131, l10: and how OVOCs

P26131, l21: “Great efforts of transport sector” is an awkward phrase.

P26132, l5: dramatic

P26133, l2: delete “random”

P26133, l16: northwest

P26135, l13: do you mean alkylcyclohexanes?

P26137, l2: previous day v

P26138, l4: I would recommend changing this sentence to read “Several additional model scenarios were constructed to test the sensitivity of simulated OVOC concentrations to assumed deposition rates and boundary layer evolution.”

P26139, l19: aromatics

P26143, l3: reflect the similarity

P26143, l20: “is of similarities” is awkward.

P26145, l24: are consistent with

P 26147: Table 5 is presented before Table 4 in the text, so their order should be switched.

Tables 2 and S1: Caption should include a definition for P(t).

Figure 4: since you subtracted background CO, the y axis labels should read ΔCO